sample of seed which had been sent from Senaar Province. This sample was sown at Ghizeh, and the material sent to Kew was taken from two adjacent plants in the same row, these being plants developed from embryos which had ripened on a tree in Senaar. The reason for sending material from two different plants was because this row showed two distinct leaf-forms, some of the plants having much wider leaf-lobes than the others, and these two types were represented by 213–1 and 213–2. In no other respect could any distinction be drawn between the two types, at least on any character within my grasp; moreover, they all flowered within the same week, ripened within the same week, grew to a remarkably uniform height, and had similar habits of growth; with the one exception of the leaf-shape they were far more alike than a similar group of plants taken from a field of any variety of Egyptian cotton. These two forms were separated by Watt into *G. arboreum* and *G. Nanking*, because "a yellow-flowered *G. arboreum* with deeply laciniate bracteoles and three glands on the under surface of the leaf would destroy the specific isolations" (p. 138). I incline to think that the laciniation of the bracteoles and the glandulation of the leaves should have been made the subject of comparative study—in order to ascertain their capacity for fluctuation—before such erratic characters were entrusted with the responsibility for this violent separation of the two forms into two separate species. Such comparative study would at least have been commenced had these plants been seen growing side by side in my plot.

On the other hand, we find on p. 181 that Moqui Indian cotton from Arizona (209–3) and "Hindi" weed cotton of Egypt (55 A) are placed together under *G. punctatum*. Waiving the query as to why Hindi, a naked-seeded cotton, should be placed in the fuzzy-seeded section, I should like on other grounds—but in all diffidence—to advance the opinion that if the two strains could be grown together at Kew, or examined side by side on my plot in Egypt, they would be systematically removed from one another by a wide interval.

The employment of common names has also been mentioned by Colonel Prain; the following instance, therefore, does not seem altogether pointless:—the plant referred to as 56.C.2 (p. 224) came from a sample of Afifi cotton, and bears lint of the brown Afifi colour; this colour is the characteristic and sole morphological distinction of Afifi from Abbassi, the latter bearing white lint, so that 56.C.2 could by no possibility be described legitimately as "close to Abbassi or Afifi."

The cultivated varieties of Egyptian cottons—and probably of Uplands—consist of many different strains mingled together and cross-fertilised, resembling one another in a few obvious characters of economic importance. Thus, on p. 224, Watt describes the strain 142, plant A, as being distinct from the Abbassi plant described in par. 2 of the same page. In point of fact, No. 142 was taken from a prize sample of Abbassi.

Though I wish to see an exact method adopted for the investigation of this labyrinthine genus, such method to be based on pedigree culture and statistical inquiry, I am nevertheless grateful to Sir George Watt for having gathered together the mass of detailed information which is to be found in his book, and I hope—with Colonel Prain—that we shall not have to wait long for the publication of further researches on the subject.

W. LAWRENCE BALLS.

Cairo, February 27.

IN the courteous letter in which Mr. Balls exercises his right to criticise details in Sir G. Watt's work on cotton, as to which he considers himself a competent judge, he gives expression to some misapprehension that it may be well to remove.

It has not been affirmed that the ideas of the writer of the review which appeared in NATURE for January 16 as to "species" and "varieties" do not accord with accepted usage. What it was necessary to point out was that the reviewer had not made it clear that his interpretation of these words accords with accepted usage. There are two passages in the review in which the words are dealt with together; in one passage they are so used as to imply that the status of a variety is the same as that of a species; in the other they are so used as to indicate that a species is subordinate in status to a variety. The ideas of the reviewer may be as precise as those of Mr. Balls; they may, on the other hand, be as loose as his own phraseology; he has given us no means of deciding.

The position assumed by me has already been explicitly stated. I have reserved perfect freedom of judgment as regards the acceptance of Watt's conclusions, not as to the limits of species in the genus Gossypium alone, but as to all the issues involved. When he explains that his general position is the reverse of this, it will be felt that Mr. Balls does himself an injustice.

The name of the distinguished public servant referred to by Mr. Balls is Mr. A. F. Broun, and is not as given in Mr. Balls' letter.

D. PRAIN.

The Isothermal Layer of the Atmosphere.

IN his letter in NATURE of February 27 Mr. Dines asks why the adiabatic conditions which prevail in the lower part of our atmosphere should suddenly cease at a height of about 40,000 feet. The answer comes more readily if the question is altered to, Why does the isothermal condition of the outer layers of our atmosphere suddenly cease at about 40,000 feet? The isothermal condition or even increased temperature with height is the condition which would naturally prevail in an atmosphere surrounding a smooth sphere. For if the sphere is a very hot one its entire gaseous envelope should acquire its temperature, whereas if the solid sphere, like our earth, is cold, and if heat from the sun is warming the atmosphere by radiation, one may expect the outer layers to be warm and the lower layers to be the coldest ones. If, however, there are irregularities, as, for instance, mountain chains on the earth's surface, then the air, whenever it is forced over them, parts with its moisture as it rises on the one side and then descends on the other side as a dry and hot *Foehn*, in which wind the conditions are perfectly adiabatic, the temperature gradient rising steadily with decreasing height. It seems, therefore, that it is our mountain ranges which prevent the isothermal condition from descending below the height at which effective mixing or moisture removing occurs.

This leads to the conclusion that if at one time our mountain ranges were lower than at present, the isothermal condition and its low temperature will also have been lower than at present. This may have been the case during Glacial periods. On the other hand, during tropical periods our mountain ranges may have been higher than they are at present; the isothermal condition will have ended at a higher level, and the steady rise of temperature below this boundary will have resulted in a very high temperature on the earth's surface.

I remember discussing this subject about twenty years ago at Aix-la-Chapelle with Dr. A. Ritter, who had only recently in Wiedemann's *Annalen* (vols. v.–viii., "Heights of Atmospheres and Conditions of Nebulæ") dealt with it very exhaustively. If I am not mistaken, it was the *Foehn* wind which had first led to these inquiries, but, strange to say, Dr. Ritter relied on molecular motions for the necessary mixing of the layers. This may have been due to his feeling that if isothermal conditions were conceded, an interstellar atmosphere would have to be postulated. We therefore almost naturally disagreed as to the possibility of condensing the so-called permanent gases, which fact had not then been accomplished. My view was that if nitrogen and oxygen should be condensable, and if the adiabatic condition existed up to the outer limits of our atmosphere, then, at the zero temperature to be found there, both gases would condense and sink to the lower levels, to be followed by further and further layers until the whole atmosphere would be deposited on the earth's surface. Dr. Ritter merely pushed this difficulty further away by saying that, even if oxygen and nitrogen could be condensed, our atmosphere might nevertheless be surrounded by hydrogen. Now that hydrogen has been condensed, helium would have to take its place, or, and this is a view not easily accepted, our earth may be surrounded by a very attenuated and possibly warm interstellar atmosphere. I think that the recent experiments

to which Mr. Dines refers by showing that the conditions of the outer atmosphere are isothermal, and Sir James Dewar's experiences with non-conducting power of high vacua are leading to the conclusion that there is a comparatively warm interstellar atmosphere.

C. E. STROMEYER.

"Lancefield," West Didsbury, March 3.

ONE would naturally expect the upper part of any large mass of fluid to be the warmer, because that condition is a possible one, whereas the converse is not possible as a permanent condition, since it involves a warmer, and therefore in general a lighter, portion of the fluid remaining under a heavier. But when dealing with a gas it is necessary to use the term "warmer" in a special sense, for which the convenient expression "potentially warmer" has been used. This means that the temperature is referred to some standard pressure, and taken as what it would be after adiabatic reduction to that pressure. In this sense the air gets rapidly warmer as we ascend, at the rate of about $0^{\circ}.4$ C. to each 100 metres, but if there were sufficient mixing we should expect to find the same potential temperature throughout, just as in a pond the heavier water is found at the bottom, but in a fast-running stream the specific gravity and the temperature are the same throughout.

We have no evidence at the present time to show how the isothermal layer is influenced by a mountain range, but there are immense stretches of sea and land so far removed from any high mountains that we can hardly suppose any such influence to exist over them.

It must be remembered that the chief heating and cooling effects on our atmosphere are applied at the bottom by contact with the ground. Pure air is almost pervious to radiation. There may be sources of heat to the upper layers; the electric currents which produce the aurora have been suggested, but I do not see that this affords any explanation of the sudden cessation of the temperature gradient.

The well-known phenomena of shooting stars apparently quite negative the suggestion of a stellar atmosphere; beside which, unless it were moving with the earth, in which case it would cease to be stellar, such an atmosphere would produce an enormously increased pressure on the forward side of the earth as it pursued its course round the sun.

W. H. DINES.

The Penetrating Radiation.

IN a letter to NATURE of February 13, the question is raised by Mr. W. W. Strong whether the larger proportion of the penetrating radiation may not arise from active matter in the air rather than in the ground. Unless the earth's supply of active matter is augmented from without, or unless it arises in a manner at present unknown, the question may be negatived, and a numerical answer given with some approach to accuracy.

Strutt has found about 3×10^{-12} grams of radium as the average amount present in 1 c.c. of soil. I have found about 10^{-16} grams of radium to be a measure of the amount of radium emanation present per c.c. of the atmosphere (*Phil. Mag.*, December, 1907). These two quantities are nearly proportional to the amounts of radium C produced per c.c. in earth and in air. The ratio is 30,000 to 1.

But McClelland and Wigger have found that the coefficients of absorption of the γ rays are proportional to the densities of the absorbers, so that the absorptions of the γ rays from radium C by soil and by air are as their densities, about 2000 to 1.

Now it has been proved (*Phil. Mag.*, September, 1906) that, for a given electroscope near the earth's surface, the penetrating radiations from earth and from air will be in the ratio Q/λ to Q'/λ', where Q, Q' are the quantities of radium per c.c. in soil and air, and λ, λ' are the coefficients of absorption of the γ rays by soil and air.

Hence the penetrating radiations from the radium C in the ground and from that in the air are in the ratio of the two ratios above stated, namely, 15 to 1.

Moreover, the radium C in the air is carried earthwards, not only by falling rain, snow, dust, or smoke, but by the potential difference in the atmosphere. The active matter on the earth's surface is thus augmented and that in the air decreased.

Observers in both hemispheres have found evidence of thorium C in the air, the activity being about half that of the radium C present. The emanation of thorium decays about 6000 times as fast as the emanation of radium, and has a poor chance of escaping from the soil, so that (1) the amount of thorium C in the ground probably exceeds the amount of radium C, and (2) the thorium C in the ground will be more than fifteen times that in the air.

We may conclude, then, that at most localities the penetrating radiation due to active matter in the air is less than one-fifteenth of that due to active matter in the earth.

A. S. EVE.

McGill University, Montreal, March 3.

Mosaic Origin of the Atomic Theory.

THE recent correspondence on the subject of the identity of the inventor of the atomic theory has led me to think that the following quotation from one of the foremost English scholars of the seventeenth century is worthy of some passing notice in this connection. Ralph Cudworth, D.D. (1617–1688), was the author of a colossal monument to Greek philosophy, the "Intellectual System of the Universe." A smaller work of that author, which was published posthumously (1731), contains the following paragraphs, which throw a glimmering light (new, probably, to most eyes) on the historic continuity of *ancient* philosophy and "modern" science:—

"1. Wherefore we have made it evident, that that very *Mechanical* or *Atomical* Philosophy, that hath been lately restored by *Cartesius* and *Gassendus*, as to the main Substance of it, was not only elder than *Epicurus*, but also than *Plato* and *Aristotle*, nay, than *Democritus* and *Leucippus* also, the commonly reputed Fathers of it. And therefore we have no Reason to discredit the report of *Posidonius* the *Stoick*, who, as *Strabo* tells us, affirmed this *Atomical* Philosophy to have been antienter than the Times of the *Trojan* War, and first to have been brought into *Greece* out of *Phenicia*. *If we may believe* Posidonius *the Stoick, the Doctrine of Atoms is antienter than the Times of the Trojan War, and was first invented and deliver'd by one* Moschus *a* Sidonian, or rather a *Phenician*, as *Sextus Empiricus* cites the Testimony of *Posidonius*. Democritus *and* Epicurus *invented the Doctrine of Atoms, unless we make that Physiology to be antienter, and derive it, as* Posidonius *the Stoick doth, from one* Moschus, *a* Phenician. And since it is certain from what we have shewed, that neither *Epicurus* nor yet *Democritus* were the first Inventors of this Physiology, this Testimony of *Posidonius* the Stoick ought in Reason to be admitted by us.

"2. Now what can be more probable than that this *Moschus* the Phenician, that *Posidonius* speaks of, is the very same Person with that *Moschus* the Physiologer, that *Jamblichus* mentions in the Life of *Pythagoras*, where he affirms, that *Pythagoras* living some time at *Sidon* in *Phenicia*, conversed with the Prophets that were the Successors of *Mochus* the Physiologer, and was instructed by them. *He conversed with the Prophets that were the Successors of* Mochus *and other* Phenician *Priests*. And what can be more certain than that both *Mochus* and *Moschus*, the *Phenician* and Philosopher, was no other than *Moses* the *Jewish* Lawgiver, as *Arcerius* rightly guesses. *It seems that it ought to be read* Moschus, *unless any had rather read it* Mochus or Moses. Wherefore according to the Antient Tradition, *Moschus* or *Moses* the *Phenician* being the First Author of the *Atomical* Philosophy, it ought to be called neither *Epicurean* nor *Democritical*, but *Moschical*, or *Mosaical*."

Dublin, February 26. JOHN KNOTT.

Tabulated Values of Certain Integrals.

IN reply to the letter of Mr. C. E. Adams in NATURE of March 19, a table of the values of the integrals required will be found in Airy's "Undulatory Theory of Optics" (Macmillan and Co., Ltd., 1877) on p. 158.

HARRY M. ELDER.

41 Netherhall Gardens, N.W., March 20.